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The Bakerian Lecture for 1809.

On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.

BY HUMPHREY DAVY, Esq. Sec. R. S. F. R. S. E. M. R. I. A.

From the Philosophical Magazine.

1. Introduction.

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries on the application of Electricity to Chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian Lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

II. *Some new Experiments on the Metals from the fixed Alkalies.*

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of enquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phenomena, different from those I adopted.

M. M. Gay Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hydrogen; a similar opinion seems to be entertained by M. Ritter. M. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hydrogen, with the alkalies; and an Inquirer† in our own country regards them as composed of *oxygen* and *hydrogen*.

I shall examine such of those notions only as have been connected with experiments, and I shall not occupy the time of the society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which M. M. Gay Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the ex-

* *Journal de Physique*, June 1808.

† Nicholson's Journal, August 1809, p. 258.

periments of the French chemists, than one given in a number of the Moniteur; and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mem. d'Arcueil, a copy of which M. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium* in ammonia, and they found that a considerable quantity of ammonia was absorbed, and hydrogen produced; and that the potassium became converted into an olive-coloured fusible substance; by heating this substance strongly, they obtained three-fifths of the ammonia again, two-fifths as ammonia, one-fifth as hydrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two-fifths, and found in the vessel in which the operation was carried on, nothing but potash.—Again, it is stated that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm that they can procure from a given quantity of ammonia, more than its volume of hydrogen.

Whence, they ask, can the hydrogen proceed? Shall it be admitted that it is from the ammonia? But this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. Berthollet, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogen gas must be produced from the metal;

* *Mem. d'Arcueil*, tom. ii. p. 309.

and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogen, and that alkali.

It is obvious, that even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygen which this would give to the potassium, and of the hydrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shown by numerous experiments, many of which have been repeated before members of this society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion is less ammonia regenerated; and I have seldom obtained as much as one tenth of the quantity absorbed. And I have never procured hydrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions, [Phil. Mag. vol. xxxiv. p. 344.] and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from three to four grains, and the absorption of the ammonia conducted as

usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases there was a considerable deficiency of hydrogen, as well as nitrogen, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases I obtained more than four-fifths of the potassium which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at 30.2in. thermometer at 54° Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7.5 were absorbed, and 3.2 of hydrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors, was exhausted, and filled with hydrogen. They contained together eight-tenths of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and one half of a cubical inch remained in the retort and adaptors. Of the nine cubical inches, one fifth of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxygen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed, was such as to generate by its action upon water three cubical inches and three-tenths of hydrogen gas.

Now if this experiment be calculated upon, it will be found, that 7·5—·2= to 7·3 of ammonia, and its electrical decomposition, would afford about 13·1 of permanent gas, containing about 3·4 of nitrogen, and 9·7 of hydrogen. But the 3·2 cubical inches of hydrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the

process, =9; and the nitrogen in the 8·8 cubical inches of gas (or the 9—·2 of ammonia) will be about three, and if we estimate ·34 of hydrogen, and ·16 of nitrogen, in the ·5, remaining in the retort, there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the eight-tenths of hydrogen pre-existing in the tube and adaptors, the loss of hydrogen will be found proportionally rather greater than that of nitrogen.

In another experiment in which three grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors, the asme quantity of residual air as in the process last described.

The 8 cubical inches of gas contained scarcely two-fifths of a cubical inch of ammonia; and the unabsorbable part detonated with oxygen, in the proportion of 11 to 6, gave a residuum of 7·5.—The barometer was at 30·2^{in}, thermometor at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys, were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6·78—· of ammonia =6·38, and this quantity of gas decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hydrogen; but there are produced in this experiment of hydrogen, 2·48 in the first operation, and 4·28 in the second, and considering the nitrogen in the permanent gas as 3·32, ·8 must be subtracted for the common air; which would give 2·53 for the nitrogen generated: and to these must be added, the quantity of hydrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hydrogen.

In all experiments of this kind, a considerable quantity of black matter separated during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort containing oxygen gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from three to four grains of potassium were made to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from four to six grains of this powder formed; but I have advanced no farther in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter which affords water by combustion in oxygen.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last bakerian lecture, and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hydrogen. When copper tubes are employed, the hydrogen bears a smaller proportion to the nitrogen, and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium of nitrogen; but a loss smaller or greater of hydrogen.

It will be asked, on what do these circumstances depend? Does the affinity of certain metals for potassium prevent it from gaining oxygen from ammonia, and do platina and copper combine with a small quantity of hydrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered

in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded but the volatile alkali, and that the *hydrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygen and hydrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

3·$1\frac{3}{10}$ grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hydrogen, and the fusible substance which was very similar to that from potassium, distilled, did not give off $_2\frac{1}{10}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and, by detonation with oxygen, proved to consist of nearly two of hydrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phenomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal, which is decompounded in this process.

As sodium does not act so violently upon oxygen, as potassium; and as soda does not absorb water from the atmosphere with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes

to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form, evidently by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies are composed of the *alkalies* merely united to charcoal, the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which M. M. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in eight cubical inches of oxygen: nearly two cubical inches of oxygen were absorbed, and soda in a state of extreme dryness, so that it could not be liquefied by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phenomena which I have described in the Bakerian lecture for 1807. Nearly six cubical inches of hydrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignated iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination; and it appears from M. Curaudau's method of decomposing the alkalies, that his metals

must have been carburets, not of potash and soda, but of potassium and sodium.

M. Ritter's argument, in favor of potassium and sodium being compounds of hydrogen, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygen than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogen; yet though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,* yet sodium is heavier than potassium in the proportion of 9 to 7, at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygen than sodium; and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxygen is given off at the positive surface, no hydrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogen than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find that instead of proving that potassium is a compound of potash and hydrogen, they confirm the idea of its being as yet like other metals undecompounded.

* *Hassenfratz, Annal. de Chim.* tome xxviii. p. 11.

When tellurium is made the positive surface in water, oxygen is given off; when it is made the negative surface, the Voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogen in water; which, in being diffused, is acted upon by the oxygen of the common air, dissolved in the water, and gradually loses a part of its hydrogen, and becomes a solid hydruret of tellurium. The compound of hydrogen and tellurium produced at the negative pole when uncombined is gaseous at common temperatures, and when muriatic acid or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large Voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which when touched by water, did not inflame nor effervesce, but rendered the water of a beautiful purple colour, and when thrown into water, entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *tellurettea* hydrogen (if the name may be used) had formed with the oxidated potassium, *i. e.* the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogen; metallic tellurium was form-

ed where it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was further confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogen was evolved by the action of the alloy upon water; but the compound of telluretted hydrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hachett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aeriform* compound of tellurium and hydrogen; I shall mention merely some of its most remarkable qualities and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hydrogen is more analagous to sulphuretted hydrogen, than to any other body. The smell of the two substances is almost precisely the same.* Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffiusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic, but which is soon converted into muriate of tellurium.†

* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, Esq. of Tunbridgé, in which Mr. Children, Mr. Pepys, and Mr. Warburton co-operated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of Voltaic electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets at the Voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogen in the Voltaic circuit. The great improbability, however, of the circumstanee that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and further researches, made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

† From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogen, en-

As arsenic has an affinity for hydrogen, it occurred to me as probable, that it would present some phenomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark-coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogen by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic* were heated together in hydrogen gas, they combined with such violence as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the Voltaic battery.

As tellurium and arsenic both combine with hydrogen, it appeared to me probable, that by the action of alloys of po-

ters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

* In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy Connais Chem. tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that though potash is decompounded in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogen. The gases not absorbable by water given off in this operation, are peculiar. Their smell is intensely fetid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogen: whether they are mixtures of various gases, or a single compound, I am not at present able to decide.

tassium, with tellurium and arsenic, upon ammonia, some new phenomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendor, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogen, instead of being pure hydrogen, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas which was principally nitrogen; so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hydrogen; which are contradictory assumptions.

None of the chymists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agents, which they did not suspect, were concerned. No person, I believe, has attempted to show that when potassium or sodium is burnt in oxygen gas, water is formed, or that water is generated when potassium decomposes any of the acids;* and no

* When in October, 1807, I obtained a dark-coloured combustible substance from boracic acid, at the negative pole in the Voltaic circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution, March 12. Soon after, I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. M.M. Gay Lussac and Thenard have likewise operated

one has been able to form potassium, by combining hydrogen with potash. I stated in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxgen gas, *the pure* alkalies were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxygen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made upon considerable portions of potassium and sodium, procured by chemical decomposition.

When potassium is burnt in trays of platina, in oxygen gas that has been dried by ignited potash, the absorption of oxygen is about $\frac{11}{20}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.* The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shown by some very well conducted inquiries, that potash and soda,† in their common state, contain a considerable proportion of water; and M. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute ex-

upon boracic acid, by potassium, and they conclude that they have decompounded it; but this does not follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash: the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

* The quantities of gas given out by the operation of water, are in a similar ratio. See page 43, of the last Bakerian lecture, [Phil. Mag. vol. xxxiii. p. 432.] and page 26 of this paper [p. 136 and 137 preceding.]

† *Annales de Chimie*, Nov. 1808, page 175.

periments, infers, that muriate of potash, which has been ignited, contains in the 100 parts 66·66 potash, and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from ten parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner: five grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light;* and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about five of hydrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

The second experiment was conducted with still more attention to minuteness. Eight grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phe-

* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

nomena as in the last experiment, and the increase of weight of the tray was $6\frac{1}{2}$ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxygen, to form 9·4 grains of potash, and 6.6—1·4—5·2, the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash; but 35.6 of muriatic acid, according to M. Berthollet's estimation, would demand 71·1 of alkali, in the state of dryness in which it exists in muriate of potash, and 71·1—64·4—6·7—so that the potash taken as a standard by M. Berthollet, contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali.*

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

* Consequently M. Berthollet's fused potash must contain nearly 23 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides: this part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

One of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia, was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chymists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists, on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data: the early discovery of Priestly on the passage of gases through red-hot tubes of earthen ware, the accurate researches of Berthollet, and the experiments of Bouillon La Grange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter

but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity.* To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxygen and hydrogen with great rapidity, and in which these gases could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gases, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubic inch; it had been carefully purged of air by the air pump, and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygen and hydrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gases; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, *i. e.* $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; six measures mixed with three measures of oxygen diminished to five; so that it consisted of 2·6 of hydrogen, and 3·4 of a gas having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hemetically sealed into the tube, it occurred to me as possible that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary

* See Dr. Pearson's elaborate experiments, on the decomposition of water by electrical explosions. Nicholson's Journal, 4to. vol. i. page 301.

communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined: six measures of it, detonated with three measures of oxygen, diminished to rather less than one measure; a result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydrogen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from those two gases alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated.* The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was

* Nicholson's Journal, August 1809, p. 258.

electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the other in an apparatus in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $_1\frac{1}{2}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases for from two or three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxygen from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though, if the asbestus was touched with unwashed hands, or the smallest particle of neutro-saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygen and hydrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid exist-

ing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made red-hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid was however generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trail in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion

of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it: figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal.* The circumstances that the ammonia ceases to be produced

* *Mem. d'Arcueil*, tom. ii. page 485.

after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestly upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{50}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times, in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxygen, $\frac{3}{16}$ hydrogen, and $\frac{6}{10}$ nitrogen.

Mr. Kirwan observed the fact, that when nitrous gas and sulphuretted hydrogen are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited, which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results; and I found that the diminution of the volume of the gases, when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears

from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that, supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogen over mercury, the barometer being at 29·5in. thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at 30·45in. and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen and hydrogen; oxygen and sulphur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompound nitrogen, by agents which I conceived might act at the same time on oxygen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen,

without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant: as soon as the contact with the potassium was made, there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygen gas.

In all trials of this kind, hydrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference that nitrogen is decompounded in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hydrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken; the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogen, nearly equal to $\frac{1}{4}$ of the nitrogen, was produced. It can-

not be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogen, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogen; but the greatest part came over densely clouded; and as nitro-muriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenor of these inquiries cannot be considered as strengthening, in any considerable degree, the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxygen of which in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which, being condensed in the upper part of the tube, would

be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubical inch of ammonia, four of nitrogen, and eight of hydrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hydrogen, and 1·1 nitrogen. The potassium produced in the operation was sufficient to generate 3·1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. Hassenfratz,* that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which, I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hydrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions,† a loss of nitrogen, and a production of hydrogen, was perceived in a case in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case

* *Journal des Mines, Avril* 1808, p. 275.
† Phil. Mag. vol. xxxiii, p. 8.

the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the naphtha, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the naphtha: and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451,* it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conductad according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance; at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both,

* Phil. Mag. vol. xxxiv, page 339.

might contain metallic matter united to oxygen: and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gases,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive-coloured matter formed on the metal; but though in this case nearly eight cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to—15°, there was a very feeble indication given of the addition of hydrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hydrogen was to the nitrogen in the average proportions of from 73·74 to 27·26; the proper corrections being made, and the precautions before referred to being taken.*

Assuming the common estimations of the specific gravity of ammonia, of hydrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807, would be supported by these new experiments; but as the moisture and oxygen visibly separated cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia, I resolved to investigate, more precisely than I had reason to think had been hitherto done, the specific gravities of the gases concerned in their dry state; and the very delicate balance belonging to the Royal institution placed the means of doing this in my power.

Nitrogen, hydrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weigh-

* Philosophical Transactions, 1809, page 459. M. Bertholett, jun. in the second volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxygen separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and at the same time he confutes some experiments which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chymist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver, which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogen may be to the hydrogen as one to three; but neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

ed. Their relative specific gravities proved to be at 30·5in. barometer, 51° Fahrenheit's thermometer.

For nitrogen, the 100 cubical inches.....29·8 grains.
For hydrogen, ditto.....2·27
For ammonia.....18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of $\frac{1}{18}$;* and if the smallest proportion be taken, the loss will be nearly $\frac{1}{12}$.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible*, that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry;

* 100 of ammonia at the rate of 185, will give 136·9 of hydrogen, weighing 3·1, and 48·1 of nitrogen, weighing 14·33 grains; but 18·4—17·4=1, and at the rate of 180, 133 of hydrogen, weighing 3·01 and 47 of nitrogen weighing 14, and 18.4—17=1·4.

and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of —20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and, when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hydrogen produced.

As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say

what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.*

If hydrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis, of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

* Even in common air, the amalgam evolves hydrogen and ammonia, nearly in these proportions, and in one experiment which I lately tried, there seemed to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rapidly absorb oxygen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly at least, from the mixture of hydrogen. Whether in any case the amalgam absorbs oxygen gas, is a question for further investigation.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at $\frac{1}{1200}$; but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56 [page 135 preceding] be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1500}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully, these ideas, and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

IV. *On the Metals of Earths.*

I have tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electro-chemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogen gas; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the

same kind as that which has been employed in the experiment.

I have passed potassium in vapor through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour, so that there is great reason to believe, that the silex had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame, was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

* The results of this experiment are opposed to the idea that potassium is a compound of hydrogen and potash or its basis; for if so, it might be expected that the hydrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination. I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogen as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapor of the water, which must have been constantly supplied.

Potassium in acting upon alumine and glucine, produced more hydrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable that even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks,* and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them; which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury, whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alu-

* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compound of potassium, sulphur and charcoal, probably contains this substance likewise.

mine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded, by solution in acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the production of an amalgam from the metals of the common earths, yet I succeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygen which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

On an hypothesis which I have before brought before the society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter, and that oxygen and inflammable bodies are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that

the attractions of acids for salifiable bases would be inversely as the quantity of oxygen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808, concluded that barytes must contain least oxygen of all the earths, and that the order as to the quantity of inflammable matter must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5* of metal per cent. strontites 86.† lime 73·5,* magnesia 66.‡

The same proportion would follow from an application of Mr. Dalton's ingenious supposition,§ that the proportion of

* Mr. James Thomson, Nicholson's Journal, 1809, p. 175, and Berthier.

† Mr. Clayfield. Thomson's Chemistry, vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol. iii. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis being inversely as the quantity of oxygen contained by the basis, though gained from the comparison of the electrical relations of the earths, with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which, in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. Gay Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygen, might (as far as it is correct) be inferred from Mr. Dalton's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxygen, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygen and of acid are always multiples of these proportions. So that if a protoxide in becoming a deutoxide takes up more acid, it will be at least double the quantity, and in these cases the oxygen will be strictly as the acid. Mr. Dalton's law even provides for cases to which M Gay Lussac's will not apply, a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron perfectly formed, (as some experiments which I have lately made seem to show,) there is probably only a single

oxygen is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, *i. e.* that every neutral salt is composed of one particle of metal, one of oxygen, and one of acid.

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition* of phospate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

M. Berzelius,† in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxygen, which agrees very nearly with the quantity of hydrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston, and though Lavoisier insisted upon this analogy with his usual sagacity, yet still the alkalies, earths, and oxides have been generally considered as separate natural orders. The earths, it has been said, are not

proportion of acid; and in the super-tartrite of potash there is only a single quantity of oxygen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

* Thompson's Chemistry, vol. ii. p. 581.

† In the same communication this able chymist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, *Mem. d'Arcueil*, tom. ii. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by M. M. Gay Lussac and Thenard.

precipitated by the triple prussiates, or by the solutions of galls;* and the alkalies and alkaline earths are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and M. Guyton) precipitate the triple prussiate of potash; the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutro-saline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and earths with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals!

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates

* *Klaproth, Annales de Chimie,* tome x. p. 277.

which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold-short iron; whilst, from another spot, it will make hot-short; but, by a combination of the two in due proportion, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Hydrogen is the body which combines with the largest proportion of oxygen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would show that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hydrogen are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shown, and as is further proved by the researches of M. M. Gay Lussac and Thenard, is a compound of a body unknown in a separate state and water. The water, I believe, cannot be decompounded, unless a new combination is formed: thus it is not changed by char-

coal ignited in the gas by Voltaic electricity; but it is decompounded by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygen being found in the new compound. This, at first view, might be supposed in favour of the idea that hydrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitro-muriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxygenation.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,* and of the boracic, fluoric, and muri-

* The electrization of sulphur and phosphorus goes far to prove that they contain combined hydrogen. From the phenomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygen, though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxygen, till I discovered that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hydrogen, and on phosphuretted hydrogen, I find that the phenomena differ very much according to the circumstances of the experiment, and in some instances I have obtained a larger volume of gas from potassium after it had been exposed to the action of certain of these bodies, than it would have given alone.

atic acids; but the relations of their compounds would lead to suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygen, or by the peculiar state of their electrical energy.

The whole tenor of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phenomena with at least equal facility.

If hydrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygen, hydrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases, with hydrogen; the earths, the fixed alkalies, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are; First, the properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hydrogen or oxygen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hydrogen, whether in its simple form, or in combination with one proportion of oxygen, or a double proportion, from my experiments seems to combine with the same quantity of

These experiments are still in progress, and I shall soon lay an account of them before the society. The idea of the existence of oxygen in sulphur and phosphorus is, however, still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium, I find, when heated in hydrogen, mixed with a small quantity of atmospheric air, absorb both oxygen and hydrogen, and become nonconducting inflammable bodies analogous to resinous ond oily substances.

alkali. Tellurium, whether in the state of oxide or of hydruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second, the facility with which metallic substances are revived, in cases in which hydrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of six inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed: the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogen is essential to the production of the metal.

Third, oxygen and hydrogen are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygen, and is restored by the addition of hydrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable that this is caused by the imperfection of the process;* and supposing potassium and sodium to produce the same quantity of hydrogen from ammonia and

* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogen evolved; *i. e.* whenever the metals of the alkalies act upon ammonia, supposing this body to be composed of three of hydrogen, and one of nitrogen, in volume, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

water, the circumstance, at first view, may be conceived favourable to the notion that they contain hydrogen, which under common circumstances of combination will be repellant to matter of the same kind: but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogen are decomposed by a metal, the quantities of hydrogen ought to be the same, or multiples of each other. Thus, in the decomposition of ammonia by potassium and sodium, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is given off; and in the action of water on potassium to form potash, the same quantity of hydrogen ought to be expelled. From my analysis* of sulphuretted hydrogen, it would appear, that if potas-

* The composition may be deduced from the experiments in the last Bakerian lecture, which show that it contains a volume of hydrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hydrogen, and 32·73 of sulphur. When sulphuretted hydrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, aud probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogen, it was found that when these gases were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common state; the phosphorus was dark coloured, and the arsenic appeared as a brown powder, both were probably hydrurets: this is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hydrogen; when the metal is in smaller quantity than is sufficient to decompose the whole of the gases, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogen contain in equal volumes, more hydrogen than sulphuretted hydrogen, probably half as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogen, it would appear that 100 cubic inches of the first, weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

sium in forming a combination with this substance sets free hydrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogen, will liberate the same proportion of hydrogen as during its solution in diluted sulphuric acid, and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogen, because, in acting upon different combinations containing hydrogen, they produce the evolution of equal proportions of this gas, then it might be proved that almost any kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve the same quantity of hydrogen, and all form dry muriates: so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogen in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygen, which may be supposed to exist in hydrogen, that might be applied to explain the composition of nitrogen from the same bases, is undoubtedly

against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenor of our chemical reasonings.

I shall not at present occupy the time of the society by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined to enable us to decide upon their nature, and their relations, or to form any general theory concerning them which is likely to be permanent.

Explanation of the Figures.

Fig. 1. The apparatus for electrizing potassium in gases. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 148. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a*, *d*, *c*, the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube of water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of 1000*l.* has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected from so great an accumulation of electrical power.

LEHMAN,

ON THE CHEMICAL PHENOMENA OF RESPIRATION.

Philadeldhia, June 26, 1811.

DEAR SIR,

IF you think the following observations possess any merit, you will oblige me by inserting them in the Baltimore Medical and Philosophical Lycæum.

Before that illustrious chemist, Priestley, discovered oxygen gas, physiologists were involved in darkness, respecting the functions of respiration. When the component parts of the atmosphere were made known, this function was closely examined. Theory, gave way to theory; and for some time there was nothing remaining but a mass of ingenious rubbish. One fact led the way to another; and in this manner, nearly every one who wrote on it, added something to its elucidation. There are few subjects that have excited more attention among philosophers, than the one under consideration. It is certainly important. The wheels of the animal machine are kept in motion by the agency of respiration. In the following observations, I shall consider the changes that take place in the blood and atmosphere during respiration; and in endeavouring to account for these phenomena, I shall also observe some of the uses of respiration. It would be an unnecessary and useless trouble, to review the different theories that have attracted the notice of the literary world.

Lavoisier's was simple and beautiful, but fell at the touch of investigation. Crawford's dwindled into obscurity before the rays of truth. Perhaps I should not have attempted to write on a subject which has been so often investigated, had not some remarks of Mr. Mitchell, in a late number of Dr. Coxe's Medical Museum, engaged me. In the first place, it is known to every physiologist, that the blood in the right side of the heart, is of a purple hue; but after circulation through the lungs is carried to the left side, of a scarlet colour. This being assumed, it remained for philosophers to explain, in what manner the change was produced. Many hyphotheses and conjectures were offered, but the generality of them were overturned by reason, and soon forgotten. The gentleman alluded to above, says, that the vermilion colour of the blood, is the consequence of the emission of carbon: but his proofs are not sufficient to support this position. If blood in any manner, be deprived of its carbon, and assume a florid appearance, is it not rational to suppose, agreeably to Mr. Mitchell, that it will not become dark unless again impregnated with carbon? This is so plain, that no one will pretend to dispute it. Will arterial blood assume a dark hue by exposure to the atmosphere? Yes. Does not this fact, then, itself, invalidate the above idea? In what manner can Mr. Mitchell reconcile it with his opinions? It is a barrier which must be removed to admit a free passage for his theory; but more facts appear to prove it erroneous. After the tourniquet has been applied; on the division of an artery, the blood will have the venous appearance. Now, certainly, according to Mr. Mitchell, it should be of a scarlet hue; for he says, it loses its carbon in the lungs, and this colour is the effect of its deprivation. If he were correct, the blood should be receiving a supply of carbon, which is not the case: because, all communication is cut off. Place florid blood in such a situation, that it can be affected neither by carbon nor oxygen, and it will take on a dark appearance: but expose it to oxygen, and it becomes again of a vermilion hue. This clearly shows that carbon is not the cause of its dark

colour. The change is more correctly, though relatively explained by the want of oxygen. The Azotic, hydrogenous and all the gases that do not contain oxygen, alter the scarlet blood to purple. How can this phenomenon occur according to Mr. Mitchell's theory? It would be absurd to say carbon had this effect. What solution, then, can be given? The oxygen must be attracted by these gases, and as the colour of the blood is altogether dependent on the presence or absence of oxygen, it assumes a purple dye. Speaking of Dr. Priestley's changing the colour of blood in a bladder, by oxygen, he observes, " If this gas could penetrate the compact texture of a bladder, and act on a fluid beneath it, why could it not also pass through the blood in a bowl and affect every part of it? I will not say whether this should be expected or not. However, I am pretty well satisfied, that the scarlet dye is not the effect of oxygen uniting with the carbon of the blood, and forming carbonic acid gas. " This, says he, " will also explain the fact, that after the surface of blood drawn out into a vessel has been rendered florid by oxygen, if the latter be removed the blood will resume its dark appearance."

The explanation of this occurrence is somewhat difficult. Does Mr. Mitchell mean the oxygen which has united with the carbon, or the oxygen of the atmosphere? If the former, it may be so; if the latter it will not; for, consistent with his theory, the carbonic acid gas already made, would sink lower, or at least being heavier than the other airs, would maintain its place: therefore it does not account for the fact. The common illustration is, that the oxygen in the blood unites more intimately with the carbon. The atmosphere, is composed, agreeably to Thompson, of about one fifth oxygen, to four fifths nitrogen. After inspiration, it is materially altered; nearly all its vital air is absorbed, and Mr. Davy says a small portion of nitrogen with it.

The question has been asked, what becomes of the oxygen? Is it discharged in the same mode it entered? There

is no necessity for this, and I think it *unrensonable* to suppose it. Besides, a portion which unites with the carbon conveyed into the blood-vessels by the lymphatics forming an oxyde of carbon, a quantity combines with the animal fibre, and is the principle from which excitability is derived. How this combination takes place, time must determine. *Animus providet futura*, is a just saying, and many of the scientific world expect that the genius and penetration of some philosopher will soon bring to light this hidden secret of nature. From what I have stated it appears, 1st. That the bright hue of the blood, is the effect of the absorbtion of oxygen. and the deficiency of this gas reasoning a priori, would be the want of this colour.

2d. To the strong attraction existing between the blood and oxygen is to be attributed the decomposition of the atmosphere. Part of the oxygen joining the oxyde of carbon in the lungs, expounds the formation of the carbonic acid gas; and the loss of a small proportion of nitrogen carried into the circulation is owing to the affinity between that gas and vital air. These facts are of the first magnitude. The wonders of respiration are yet open to inquiry. Who knows but La Grange's theory ere long may meet the fate of its predecessors? So fluctuating is the science of chemistry, that we can scarcely affirm any thing is, or is not correct. Every new opinion should be examined, and error be detected in its infancy. I shall now make a few remarks on my last proposition, viz. The uses of respiration. When we see that every animal in nature, has respiratory organs we may with propriety conclude, that this function is indispensably necessary to life.

1. It is impossible for blood not possessing the principle of vitality to sustain the human body. Respiration performs the noble office of affording this principle which is oxygen.

2. It was for some time thought that the union of oxygen with the blood was the only source of animal heat; this Dr. Rush has long since shewn, to be erroneous. It generates

heat, so does almost every operation in the animal economy.

3. It gives a scarlet dye to the blood and without oxygen life would soon cease.

4. It conveys out of the system, gases which are known to be deleterious.—

5. The air inhaled increases the irritability, of the muscles.

6. It assists in forming the human voice, by which man is so much exalted above the brute creation. Many other uses of respiration might be mentioned, but these are sufficient to shew the connexion between it and life.

With respect I am yours

GEORGE F. LEHMAN.

.....

Hall on the Influenza of 1807.

Baltimore, 2d July, 1811.

SIR,

IT is deemed unnecessary to offer an apology for the history of an epidemic, however deficient it may be in arrangement, provided it can add by a single fact to the general stock of medical knowledge.

Enclosed you will receive a short account of the influenza of 1807, as it appeared in the upper section of Harford county, (Maryland,) about N. latitude 39, 40, and 1 E. longitude. Provided you think proper you are at liberty to insert it in the Lycæum.

The influenza made its appearance in the abovementioned section of country about the last of August, and advanced with a rapidity unequalled by any other epidemic. The temperature of the weather was rather more cool and dry, than usually occurs at this season. The thermometer generally ranged between 70 and 83, of Fahrenheit, seldom exceeding 85.—

The disease passed by children (with a few exceptions) and attacked those of both sexes with equal violence who were arriving at, or had attained the age of puberty. Persons advanced in life who had been debilitated by disease, or any other cause, suffered extremely by its visitation.

The *premonitory* symptoms were violent and long continued sneezing, (indicating an irrtaition of the Schneiderian membrane), hoarseness, slight cough and titillation about the superior portion of the trachea; and an obtuse pain of the head and muscles of the neck, back, and inferior extremities. An accute pain over the frontal sinuses, or glassy appearance of the eyes often indicated the approach of this unwelcome visitor.—

The above symptoms were succeeded by a rigor or chill of different degrees of violence, followed by a fever of the inflammatory grade. A perspiration sometimes supervened and often afforded relief.

In some persons all the above symptoms shewed themselves; in others a few only occurred.

In a generality of cases, the cough was the most distressing symptom; and in aged persons, occasionally equalled the pertussis in violence and duration.

By many, acute pains were felt darting through the thorax, more particularly during the paroxysm of coughing; and in several instances which came under my care severe pleurisies ensued.

It produced pneumonia notha in a few persons who had previously laboured under pulmonary desease, and suddenly proved fatal.

A tumefaction of the amygdalæ was not an uncommon symptom, and it subsided with the hoareness which was generally removed by expectoration.

The fever evidently partook of the intermittent type and had its exacerbations generally in the evening. The pulse was often full and tense, more generally moderately so; sometimes natural, but seldom below the usual standard; except when depressed as in pneumonia notha.

The tongue exhibited (with few exceptions) more or less, of the white or febrile appearance, according to the severity of the disease.

The bowels were generally costive; in a few persons of the intestinal temperament a diarrhœa took place.

The fever seldom continued more than four or five days, unless in those cases where the lungs were much affected.

The disease began to decline in four or five weeks from its first appearance, but left many afflicted with a troublesome catarrh.

The mode of treatment, in this epidemic varied according to the symptoms; when the pulse was full and tense, the lungs oppressed, breathing difficult, and acute pains were felt in the thorax, venæsection was resorted to with immediate advantage; and in those cases which terminated in pleurisy the lancet was freely used; often twice a day. Here also other auxiliaries were thrown in, as emetics, vesicatories, and such remedies as are generally termed antiphlogistic. The loss of blood moderated the fever, relieved the pains of the thorax, head and extremities, and palliated the cough. It was repeated according to the pulse and the violence of the symptoms, and in no case proved injurious. Many persons prescribed it in their families without consulting a physician; even when the severity of the symptoms did not appear to require it. As far as my information extended no ill consequences were the result. I mention this more particularly, because in the influenza of 1790, Dr. Rush remarks that in several cases blood-letting produced alarming results.

Demulcent drinks were beneficial throughout every stage of the disease, by removing the disposition to frequent coughing which almost uniformly attended.

But no article of the materia medica in my hands afforded such speedy and obvious relief to this distressing symptom as * digitalis which was generally exhibited in the form of

* During the prevalence of this epidemic in Baltimore, it generally happened that the cough outlived all the other symptoms.

tincture. It acted as by incantation in relieving the cough, and when taken at night procured undisturbed repose. The tinctura opii was occasionally added to the digitalis, but the latter acted as an anodyne, when the former failed. From the extensive manner in which I administered the digitalis, and the great relief which it uniformly afforded, I am induced to consider it as a most valuable article in the treatment of the influenza.

Long after the subsidence of general fever a chronic inflammation of the lungs and their appendages, the bronchiæ and trachea required the physicians attention. It often resisted blisters, anodynes, emetics, diaphoreties, and all the demulcents usually resorted to in pulmonary affections; but the tincture of digitalis succeeded radically in every instance, except one, which terminated in phthysis pulmonalis from the neglect of bloodletting.

Several writers have derogated from the character of the lancet in this epidemic, or in others that have received the same appellation. Whatever grade of inflammation may have constituted this disease in other places or at other times, the following facts are vehement presumptions, that the most prominent error in its treatment was the parsimonious use of this noble antiphlogistic.

1st. Notwithstanding the disease, when left to nature or a temporizing treatment always observed one uniform, protracted routine; bloodletting employed early and copiously always abridged its duration, and often obviated the vexation of a tedious expectoration.

2d. It often prevented the cough, and rendered a secondary treatment with the digitalis unnecessary.

3d It prevented that slow convalescence so frequently the consequence of reiterated pulmonary irritation.

4th. In several instances a spontaneous hemorrhage from the nose, far more copious then the artificial hemorrhage from the arm, put a period to the disease in a time much shorter than that required by the ordinary use of the lancet. On the 13th of August, Daniel Steuart an athletic man aged thirty eight, was attacked with the prevailing epidemic. About twenty eight hours from the accession of the chill, I was called to arrest a hemorrhage from his nose. He had discharged about four pounds of blood

Purgatives were seldom administered with advantage. Gentle laxatives only were used to obviate constipation.

Hard labour, or violent and long continued exertion removed the disease in a few cases: In my own, severe exercise on horse back effected a cure.

and still the pulses were tense, and the symptoms only mitigated. I suffered the blood to flow till nearly seven pounds had been avacuated, when a compress was applied to the bleeding vessel and the hemorrhage ceased. Although this was one of the most formidable attacks that occurred to me, it was the soonest cured. In forty-two hours from its commencement it was completely quelled, and the convalescence was quick and uninterrupted, except from the voracious appetite of the patient, which required to be restrained. This case is stated merely as an illustration of the principle; not because it is solitary. There were several very delicate subjects, who recovered rapidly after hemorrhagies from the nose, far more abundant than the most sanguinary bleeder would have been willing to draw from the arm, n a disease whose aspect was so little imposing as not to assume the most prominent characteristics of inflammation.

There was one more fact connected with this subject, that may one day reflect light on the remote cause of the influenza or the pathology of asthma. At the commencement of the epidemic, several persons who were suffering under an asthmatic paroxysm were suddenly relieved: their respiration became natural and easy, while the lungs seemed to have resumed their healthy functions. The influenza began to decline in about three weeks from its first appearance; and during this time no asthmatic person was visited by a paroxysm. Nor did any of them experience the slightest symptom of the epidemic, which pervaded all other ages, sexes and conditions of adults. This jubilee to afflicted humanity was however but fleeting, for no sooner had the monarch of the day deposed his authority, then the asthma was reverberated with more than usual violence. Four persons who were under my care testified to these facts, but as it was still possible that accidental coincidence might have accounted for them, I resorted to other testimony and found that eight more asthmatics concurred with my own patients.

Ed.

Many hundreds who were attacked by it, continued to pursue their usual occupations; others who kept their houses were not confined to bed. In many it proved so mild as scarcely to demand attention.

The appetite often continued unimpaired, particularly when the disease was not violent in its symptoms.

It is a fact confirmed by the observation of the inhabitants, that bilious remittent and intermittent fevers which are endemial in this district of country, (from its contiguity to the river Susquehanna, and a considerable creek), were never so rare as during the continuance of the influenza. The autumn after its prevalence was unusually exempted from the autumnal forms of fever.

It has been the opinion of some physicians of eminence, that the influenza is a contagious disease, but the belief that it is only propagated by contagion is invalidated by the fact, that several years would be required, to spread it in this manner as extensively, as observation teaches us, it has been propagated in a few weeks. That it does not depend altogether on the qualities of the atmosphere, is evident from its appearance at all the various seasons of the year, and by the fact that it extends its ungracious influence with equal, and almost electrical rapidity, over countries which are congealed by the cold of a northern winter, and those which are schorched by the ardent rays of a vertical sun. May we not venture an opinion, that a comparison of the histories of this disease which have already been given to the world, with those which posterity may produce, will hereafter afford a clue, by which a knowledge of its phenomena may be obtained?

RICHARD W. HALL.

WILLIAMSON

On the connection between Scrofula and phthysis Pulmonalis

Baltimore, May 30th, 1811.

IN my last I concluded my remarks directly relative to scrofula. I shall now proceed to give my sentiments on this disease in conjunction with phthysis pulmonalis.

April 4th, 1808.

I am now attending a patient in the last stage of phthysis pulmonalis, whom I first saw about six months ago, and who has strumous tumors of considerable size on the neck, evidently of a scrofulous nature. These tumors are of several years duration.

This fact has, I conceive, a tendency to prove that there is some truth in the old opinion of a relation between the scrofula and phthysis pulmonalis. Dr. Mead has observed, "*that those persons are most liable to ulcers in the lungs, who have strumous swellings in childhood, or youth;* and he adds; "that most experienced physician Ratcliff, used to say, that most consumptions in England and in other cold countries, were of the strumous kind." We often see in dissecting the bodies of patients, who have died of consumption, the lungs filled with tumors or indurated glands, which had suppurated and discharged matter. Although I believe in the probability of the phthysis pulmonalis being frequently, and perhaps in all hereditary cases, ingrafted on scrofulous habits; yet, I am very far from believing that persons of this class or habit, are the only ones subject to that disease.

When separate, these diseases are either of them formidable in their nature, and frequently desperate in their event: when combined they must be doubly so!

If there be different species of the phthysis pulmonalis, it is rational to suppose that different methods of treatment should be adopted; and in order to accomplish the desired

effect, it is the incumbent duty of every practitioner of medicine, to endeavour to distinguish between different forms or species of the same disease. For, to endeavour to cure the phthysis of a scrofulous nature, by remedies which are proper in that of an inflammatory or pneumonic order, would be as irrational, as it would be to attempt to extinguish a fire by adding fuel. Although the scrofula has generally been considered an incurable disease; yet, fortunate experience has taught us that it may sometimes be cured. If it be possible to cure it when affecting the glands perceptible to the eye, may we not flatter ourselves, that it is also curable when seated in these glands situated in the interior of the system?

Facts are incontrovertible, and they have proved that the phthysis is sometimes of an inflammatory nature, and only to be remedied by the antiphlogistic treatment. But facts have never proved the antiphlogistic treatment salutary in scrofula. On the contrary, they have proved it exceedingly pernicious. If therefore the consumption is ever of a scrofulous nature, and facts have proved the antiphlogistic treatment improper in scrofula, analogy should teach us the impropriety of adopting this mode of treatment in diseases of that nature. Supposing then that there are these two different forms of phthysis, and granting that our opinion respecting the different method of treatment be correct, we can very easily perceive, that by mistaking or confounding them, incalculable injuries may arise; and that all the anxiety which a sympathetic mind can possess, and all the ardent wishes that humanity can excite, with our greatest exertions and strictest attention, must prove abortive. When, on the contrary were we able to distinguish completely between the different forms of disease, and prescribe according to their nature, we might, perhaps not unfrequently have our solicitude crowned with a fortunate issue.

It was formerly believed that the phthysis pulmonalis was an incurable disease; and whatever physicians of great eminence of the present day, may have asserted to the contra-

ry, it is yet doubtful with me, whether a confirmed phthysis can ever be cured.

The situation of the lungs, with their important and active functions render it very difficult to cure them when diseased, especially if the disease be of an extensive or chronic nature. It is in consequence of this, with that of confounding or mistaking the different species, and consequently treating it improperly, that the consumption remains, as it ever has been, almost always incurable. But in this enlightened age, when the rays of phylosophy are radiating from their centre, and unveiling to the industrious student truths before unknown; (many of which have been discovered in chemistry, and not a few in the science of medicine) may we not rationally hope, that she will be still more bountiful, and continue to guide us by her divine light into the mysterious paths of our profession, and illuminate them so as to enable us to discriminate more completely between diseases of a different nature or different species of the same disease, and teach us by her laws how to prescribe for them, when known. Although we dare hope for an event of this invaluable nature; yet we dare not anticipate that day, when the consumption will not be so formidable a disease as to frequently terminate fatally.

> "Though skilfully, the sage perform his part,
> It will oft tryumph o'er the healing art."

The grand improvements or discoveries which I anticipate relative to the phthysis pulmonalis, are more of a preventive than a curative nature.

As I do not intend to dilate on the consumption in general, the subsequent remarks will be principally confined to that of a scrofulous nature.

If the scrofula is difficult to cure, when the glands of the neck, &c. are the seat of disease, it must be much more so, when seated in the lungs. For independent of the difficulties attending the treatment of this disease in general, there are additional ones, when the lungs are the seat of it.

The bath, as mentioned in my former essay, I consider very beneficial in scrofula, when the lungs are not the seat of disease, but when they are, it is always a doubtful remedy, if not a pernicious practice. I have tried it in the cholera infantum, some months after the tussis convulsiva, and when this last disease had apparently entirely disappeared; but it excited a morbid action in the lungs, and we were compelled to discontinue it it as pernicious. Other cases might be adduced to prove its injurious effects in most diseases of the lungs attented with cough, or where there is a great predisposition to affections of this nature, but this is a fact too well known to need dilating on by me.

It is possible the tepid bath might be used with advantage in scrofulous affections of the lungs, as the cutis generally sympathises with the diseased organ. While some parts of it are cold, others are preternaturally warm; it is however most generally parched and dry. The cold bath, by carrying off the heat too rapidly, causes a contraction of the skin and increases the flow of blood to the heart and lungs; whereas the tepid bath, if the skin be cold, would produce a determination to the superficies, and if parched would tend to equalize the animal heat, and produce a salutary perspiration. However great care is requisite in using this bath, if it be too warm, it will increase the general excitement, and may be productive of much injury. I am induced to believe that the air bath with friction, properly conducted, would be highly salutary in scrofulous affections of the lungs.

The use of external or local remedies, I think very salutary in ordinary cases of scrofula; they can however be had access to but very imperfectly, if at all, when the lungs are the seat of the disease. Those medicines inhaled should, I suppose, be considered of the local order.

Dr. Pearson of Birmingham, has spoken in exalted terms of the good effects of ether and cicuta, when inhaled, in the phthysis pulmonalis. If this compound is ever salutary, I suspect, it must be in the scrofulous species of this di-

sease. The Doctor adds one scruple or half a drachm of the powdered leaves to every ounce of sulphuric ether. After the ether becomes sufficiently impregnated, one or two tea spoonsfull are put in a wine glass or cup, then the patient holds it to his mouth and inspires the vapour that arises from it, until the ether is evaporated. This is repeated three or four times a day.

The first effects of this application, says the doctor, "are, an agreeable sensation of coolness in the chest, an abatement of the dyspnæa and cough, and after ten minutes or a quarter of an hour, easier expectoration." The doctor then proceeds to say, what the ultimate effects are, but here he has recourse to conjecture. Other proper measures are not to be neglected as this is not to supersede the use of other medicines. After mentioning what the ultimate effects are, he proceeds: "to assert that all these beneficial consequences will flow from its application in every species and degree of the phthysis pulmonalis would be adopting the language of quacks, and insulting the understanding of every one experienced in the profession."

From this, and the following sentence, the doctor must have been at least, in part aware of its being limited in its effects. I shall only remark, says he, "that it appears to be best suited to the florid, or what is termed the scrofulous consumption." He tried the ether with several volatile substances, but the cicuta was the most efficacious. "The narcotic particles of the cicuta, conveyed in this manner, along with the ether, to the diseased lungs, act as a topical application with the best effect."

The cicuta we know has long been celebrated in scrofula. The ether, no doubt increases the exhalation from the diseased parts, and carries off the superabundant heat, the sensation experienced warrants this conclusion. This remedy may also operate as a local stimulus and excite the absorbents to an increased action.

The incipient or first stage of the phthysis pulmonalis is that, in which this medicine must have the most benign ef-

fect. Dr. Pearson, it is probable, never used it except in confirmed cases; and experience taught him it was inefficacious in the last stage of the disease, or in complicated affections, as he thus observes. "Where the pulmonic affection is complicated with mesenteric obstructions or diseases of the other viscera, or a dropsical condition, it affords but transitory relief; and in the very last stage of the disease the proper time of using it is past."

But the two most forcible causes why this disease is more fatal when the lungs are the seat of it, than when the external glands are; are first, the peculiar and active situation of the diseased organs. Second, the difficulty, or almost impossibility of timely discovering the disease. I have no doubt, but that the lungs may be in a morbid state a long time, without being suspected of disease.

The first of these reasons is so strikingly obvious that it were useless to dwell on it, further than to observe the necessity of studiously attending to the various diseases to which the lungs are subject, to be always careful to distinguish them, and to prescribe for the symptoms of disease and not for its nosological name.

In diseases of the lungs, all the minutiæ should be strictly attended to, not only respecting its nature and treatment, but also relative to the patients constitution, habits, family, present and former place of residence, diet, clothing, occupation, and every thing that can have any influence on the disease or mode of treatment.

Second, the difficulty or almost impossibility of timely discovering the disease. Fernelius justly remarks, when speaking of a vomica of the lungs. "This is a very obscure and concealed disease, and often not apparent either to the patient or physician; so that the patient neither alters his course of living, nor thinks himself ill, but bears about unknowingly in his breast the seeds of death." That this is the case in scrofulous affections must be obvious to every person acquainted with the nature of the lungs, as well as of this disease. Therefore, when applied to by a patient

who has a morbid affection of this viscus, we should be particularly careful to ascertain the nature of the disease; and if the symptoms, with the general constitution of the patient indicate a disease of this nature, we should treat it as such. The treatment proper for scrofula in general will be proper in this case, with a few exceptions, and these a judicious practioner will be able to detect. But the treatment proper for a pneumonic phthysis would be very pernicious in this species of consumption.

Bloodletting, mercury, &c. which have been proved by our venerable countryman, Rush, to be salutary in many cases of phthysis, must be injurious in that of a scrofulous nature, tending to debilitate the general system, and all debilitating remedies are improper in scrofula. When this disease is evidently about to terminate in the phthysis, if there be a sense of engorgement or fulness in the lungs, a few ounces of blood may be drawn, but let this be done with judgment and not without sufficient indication.

If the preparations of mercury are ever beneficial in this species of disease, it is only when given in small quantities, and combined with tonics. For, if it is continued until it has a general effect on the system, it debilitates, and, I believe, tends to produce suppuration in the scrofulous tumors; and when these tumors suppurate, they are much more difficultly, cured. Hence the necessity of distinguishing between scrofulous and other schirrous tumors, for while mercurials are very pernicious in the former, they are highly salutary in the latter. I know of no better way of distinguishing these diseases, than by consulting the constitution of the patient.

In addition to the indication of the patient's constitution, there are distinguishing symptoms between the scrofula and other tumors, so that no physician who has had much practice, will either mistake or confound them. The viscera sometimes become scirrhous after active inflammation. The exterior glands are also frequently enlarged, and sometimes put on a scirrhous form of disease. There are also cold

indolent tumors, which appear on the exterior of the body, over different parts. Such as the steatomatous, &c. However, there is so little analogy between these diseases, that the practitioner must be ignorant indeed, who would confound them.

The steatomatous, &c. may be extirpated without danger and to the relief of the patient, but the scrofulous cannot.

The bulk of the steatomatous, &c. is the principal inconvenience to be apprehended from them, for it is well known, that they sometimes grow to an enormous size without occasioning any alarming symptom; but the scrofulous tumor is not the only inconvenience, that a patient labouring under this disease experiences. Indeed the tumor appears to be only an indication of a disease of the general system. Before any symptom of suppuration is discovered; in fact as soon as the gland begins to enlarge, or even before there are symptoms of an enlargement, a disease of the general system is obvious. The skin becomes lax, the solids relaxed, there is a loss of appetite, dyspeptic symptoms, often supervene; and these affections continue to increase, unless remedied, until a universal cachexy prevails, and the patient gradually declines until death removes him from this scene of affliction.

In scrofula in general the digitalis is certainly pernicious, but in that of the lungs it may be beneficial from the immediate influence it has on the pulse. But it should be used with great care, and if it does not reduce the action of the pulse, it should be discarded as injurious, as it reduces the tone of the general system. If it should reduce the action of the pulse, it will enable us to administer, more freely those medicines which we must have recourse to for the cure of this disease.

(To be continued.)

A Case of Artificial dilatation of the Female Urethra, &c. &c.

BY H. L. THOMAS, ESQ. F. R. S.

From the Medico-Chirurgical Transactions for 1809.

SUNDAY, April 26th, I was called into the country to see a lady, thirty-four years of age, and the mother of several children. On the morning of that day she had, without any assignable cause, laboured under a suppression of urine; as there was no regular assistance at hand, the husband (whose readings on this subject had been pretty extensive,) took upon himself to afford her relief. With this view he introduced an ivory earpicker into the meatus urinarius, and the water immediately flowed, but as the quantity evacuated did not quite equal his expectations, the instrument was introduced a second time, when it escaped from his fingers, and suddenly slipt, as he expressed it, with a "jerk into the body." When I saw her, six hours after, she was free from pain, and with no other unpleasant symptom than those which might be expected from the agitated state of her mind. Upon introducing the sound the extraneous substance was readily detected, and was as easily laid hold of by a pair of fine polypus forceps, but I found every attempt to extract it was attended with considerable pain, and followed by a slight discharge of blood, appearances which gave me reason to imagine that the instrument lay across the bladder, with its pointed extremities entangled between the fasciculi of muscular fibres, and that much mischief might be produced by any farther attempts at extraction. I therefore merely ordered an enema to be administered, and directed her to drink plentifully of diluting mucilaginous liquids; I also gave some directions respecting the position of the body at the time of evacuating the urine, which were to be such as to render the orifice of the bladder the most depending part.

On the Monday at two o'clock, I found that she had passed a restless night, and was much indisposed; pulse frequent, face flushed, and the tongue furred; a slight degree of ten-

derness was beginning to pervade the whole of the hypogastric region. This situation of the patient clearly pointed out the propriety of a speedy removal of the cause of irritation, and as I came provided with the bistoire cachee, I proposed opening into the bladder immediately; this proposition was, however, absolutely rejected by the husband, from the knowledge he had of a case of lithotomy in a female, who ever afterwards suffered from an incontinency of urine.

With some hesitation, and I must confess with little prospect of success, I introduced a piece of sponge tent into the urethra, three inches long, and somewhat thicker than the full sized female sound, leaving directions for its removal at the end of two hours, or even sooner, if attended with much pain or distress. I took care to prevent the possibility of the sponge separating, and any part escaping into the bladder, by passing a strong string through its whole length secured at both extremities by a double knot.

In the evening I was informed that she had borne the tent for two hours, without any great degree of irritation taking place, and that during the whole time the urine gradually escaped. I sent another piece of the tent somewhat larger than the first, desiring it to be introduced at twelve o'clock the following morning, and allowed to remain in the urethra till I came to her at two. On my arrival I found her tolerable well, and upon withdrawing the tent I passed the forefinger of my left hand into the bladder, where I felt the ear-picker lying across the cervix. At first I conceived it impossible, without laceration to disengage it from the position in which it appeared so firmly fixed, but by turning my hand, and insinuating the point of the fore finger underneath, and towards the blunt extremity, it was readily dislodged, and escaped through the urethra, by the side of my finger. It measured three inches in length, one end being considerably more pointed than the other. I kept my finger engaged in the bladder for (I should guess) five minutes after the expulsion of the ear-picker, solely with a view of attending to the contractile force of that viscus. The sensations I ex-

perienced did not in any way accord with the ideas I had formed, for instead of my finger being firmly compressed, as I expected, by the sphincter muscle and bladder generally, I found every where nothing else than a soft pulpy yielding substance, totally insensible to any stimulus I could produce by the finger nail; this inability to contract, may perhaps be accounted for by the partial distention of the bladder for so long a time by the ear-picker. The same agent, however, cannot be assigned as the cause of the general relaxation which had taken place in the urethra, and which was so very complete, that before I withdrew my finger, I believe, had the case required it, both thumb and finger would have passed into the bladder without the smallest difficulty.

On the following day the patient was so well as to preclude even an excuse for any farther examination of the parts; but I was given to understand that the involuntary discharge of urine continued only six hours, and that now she possessed the full powers of expelling it without any other inconveniency than a slight degree of scalding in its passage along the urethra.

My view in laying this case before the Society is to show how readily, and with what little pain the female urethra will admit of dilatation. There is no novelty in the mode of accomplishing the object of distention. Near a century back it was proposed by Douglas; he not only recommended sponge for this purpose, but also dried gentian root, as being more gradual in its expansion, and better fitted for the purpose.

Mr. Bloomfield has given the case of a young girl where he effected the dilatation by introducing the cœcum of a small animal, in a collapsed state, into the bladder, filling it afterwards with warm water, by means of a syringe. This was gradually withdrawn as the cervix vesicæ opened, and in a few hours the dilatation was so far accomplished as to allow the calculus to pass through.

Why some of these methods have not been more generally adopted I cannot say, perhaps the incontinency of urine which occasionally has been observed to succeed to great distention of the urethra is the reason of their having been laid aside; with what propriety, however, may be questioned, for I believe it will be found that this unpleasant symptom frequently occurs after the operation of lithotomy, as it is now usually performed.

We have many well authenticated cases on record where calculi, of a size larger than a hen's egg, have been expelled from the bladder by the expulsive efforts of its own muscles. Heister has related from good authority several instances of the kind, a very remarkable case is also given by Dr. Molineux, in the early part of the Philosophical Transactions, where a stone was voided by a woman, "the circumference of which measured the longest way $7\frac{6}{10}$ inches, and round about where it was thickest $5\frac{3}{4}$ inches, its weight near $2\frac{1}{2}$ ounces troy.".

If these relations can be credited, and there is no reason why they should not, I can hardly conceive any cause in a young and healthy female subject, and where the bladder is free from disease, why a very large stone may not be extracted without the use of any other instrument than the forceps, the urethra having first been sufficiently dilated by means of the sponge tents; for this purpose the plates of the forceps need not be so thick and strong as those commonly employed.

It may be often observed, that muscular fibres, when once stretched to a certain extent, (i. e.) beyond their capability of immediately reacting, will admit of very considerable elongation without tearing, and yet shall, when the cause is removed, perfectly and very speedily recover their original powers. When the luxation of the femur takes place downwards, we observe the great gluteus muscle upon the stretch, almost to breaking: before the reduction can be accomplished the fibres will be still farther elongated, yet after this violence of extension, laceration of the fibres hardly

ever happens, and when the muscles are again restored to their former situation, their functions become as perfect in a few days as if no injury had been sustained. That phincter muscles are possessed of similar powers, the above case will, I think, in great measure prove, in addition to which I beg leave to subjoin the following.

A gentleman of an inactive and sedentary disposition had for many years suffered from constipated bowels, which increased to that degree that the most active cathartics failed in producing the desired effect. By the advice of a practitioner, whom he consulted in Paris, he daily introduced into the rectum a piece of flexible cane (about a finger's thickness,) where it was allowed to remain until the desire for evacuating the fæces came on. This plan succeeded so well that for more than a twelve month he never had occasion to resort to any other means. One morning, being anxious to fulfil a particular engagement in good time, in his hurry he passed the stick farther up, and with less caution than usual, when it was suddenly sucked up into the body, beyond the reach of his fingers. This accident, however, did not interrupt the free discharge of the fæces, and the same evacuation regularly took place every day, whilst the stick remained in the gut. It was seven days afterwards when I first saw him; he was in a very distressed state, with every symptom of fever, tension of the abdomen, and a countenance expressive of the greatest anxiety. His relatives and friends were totally ignorant of the real nature of his case; and nothing less than the urgency of his sufferings, could ever have prevailed upon him to disclose it to me. Such were his feelings on the occasion, that a violent hysteric fit was brought on by the mere recital of what he termed his folly.

Upon examination with my finger, per anum, no part of the cane could be discovered; but one end of it was readily felt projecting (as it were) through the parietes of the ab-

domen, midway between the ilium and the umbilicus on the right side. The slightet presssure upon this part gave him exquisite pain.

After repeated trials I was at length enabled, with a bougie to feel one extremity of the stick lodged high up in the rectum; but without being able to lay hold of it with the stone forceps. To allay the irritation for the present, an emollient clyster, with tinct. opii. 3ij, was given, which passed without the least impediment, and did not return. On the next examination, two hours after, I found the phincter ani considerably dilated, and by a continued perseverance to increase it, the relaxation became so complete, that in about twenty minutes I was enabled to introduce one finger after the other, until the whole hand was engaged in the rectum.

I found the end of the stick jammed in the hollow of the sacrum, but by bending the body forward it was readily disengaged, and extracted. Its length was nine inches and an half, with one extremity very ragged and uneven.

For several days after, the situation of the patient was highly critical, the local injury, joined to the perturbation of his mind, brought on symptoms truly alarming. At length I had the satisfaction to witness his complete recovery; and he has ever since (more than two years ago) enjoyed good health, and the regular action of the bowels, without the assistance of medicines, or any other aid.

Haslam on the Locked-jaw in Horses.

Baltimore, 2d July, 1811.

To the Editor of the Lycaeum.

SIR,

I HOPE it will not be considered disrespectful to the faculty, or presumption in me, to state a few facts and opinions respecting the *locked-jaw* of horses. This useful animal is much oftener affected with tetanus than is generally supposed, and it has almost in every instance ended in death. Although I think I can say without vanity, that I have been successful in other diseases of the equine race, I candidly confess I never succeeded in a case of this dreadful malady till you recommended the use of calomel. I had prescribed opium, brandy, camphor, spirit of turpentine, and many other strong stimulants, but to no good purpose.

As all your readers may not be acquainted with the symptoms of this disease in horses, I will state them as they appeared to me in those cases that fell under my observation.

The nictating membrane (which in a state of nature is but partially to be seen) becomes conspicuous; the nose is pointed horizontally, and the tail raised considerably and is turned a little to one side. The horse ruminates his food like a cow, scarcely separating his jaws, and his walk is much changed; both his fore and hind legs being much farther apart than usual. His flesh becomes very hard, and at last his jaws are completely locked. This is commonly the last symptom, but in some cases all these symptoms appear nearly at the same time. When this is the case I consider it as an unfavourable sign; for the slower the symptoms succeed to each other the less dangerous the disease and vice versa, for when the affection of the jaws soon succeeds the other symptoms there is the greatest danger.

One of the greatest impediments to the cure of this disease is the vulgar error of supposing that the horses have what are called the *hooks*, an opinion that originated in igno-

rance, because it has no foundation in nature. To prevent the cruel operation of cutting for this supposed disease, (which would be worse then the disease if it existed at all) I shall give a description of this part of the eye,—The horse like all other animals being liable to the introduction of foreign matters into the eye, and not being favoured like man with hands or other means of removing them, has been bountifully furnished by nature with a nictating membrane. This is of a strong caitilaginous nature, and is put in motion by the retractor muscle drawing the eye into the orbit, by which motion this membrane is brought over the globe of the eye. Thus, by the motion of the eye it serves to cleanse or wipe off any offensive matter that may have been introduced.

I have met with some well informed people as to other subjects who believed that this membrane had arisen there in the course of a few days, because they were not acquainted with the causes that rendered it more visible than usual. There are many such causes, but more especially the two following. 1st. Inflammation in the eye, which when stimulated by the rays of light is drawn into the orbit, leaving the membrane partially exposed. 2d. Spasms of the muscles of the eye or parts adjacent which produce the same effect, and is the reason why, the tetanus is so often supposed to be the hooks which have no existence. This mistake is often fatal; for I have sometimes been unable to persuade the owners of horses of their deception and the cartilage has been cut out to the destruction of the poor animal. Such is the prevailing opinion on this subject, that as soon as a horse is sick of any complaint which they do not understand (and this is generally the case) they begin to look for something external that will enable them to account for the symptoms, and this is often very striking in the eye, because in many diseases this membrane is more than usually obvious for the reasons already assigned. The operation is immediately performed, and if the horse does not die of inflammation or a locked jaw, he generally becomes blind, and will be so for life.

Of the causes that give rise to this disease in horses I have observed injuries of the feet (more especially from nails) to be the most common. There seems to be something in iron nails, that oftener occasions tetanus than any thing else. Eight out of the last ten cases under my care were brought on by this cause. It is nevertheless sometimes brought on by wounds from other causes. I have seen a case produced by docking the tail, and I cannot forbear to observe here that I think strong stimulating dressings after certain operations, tend to produce this effect. I have also seen it brought on by an operation which was followed by a high degree of inflammation. On the 7th of June, 1810, I removed a large tumor from the scrotum of a mule. It had become so large as to be very inconvenient. After the operation there was a very free discharge and every thing looked well; but on the 19th, I was sent for, and found that he laboured under a complete tetanus. Although it had appeared only a few hours, he died before morning. I opened the body the next morning and found, that the inflammation had communicated to the intestines and stomack, and that he had died of mortification.

I have conceived that there are two species of tetanus very different from each other; the one evidently arising from great and general debility giving rise to spasms, and the other originating from excess of inflammation requiring evacuations for its cure. The above case shews at least that either the tetanus arose from inflammation, or was present at the same time, and I think we must conclude they were connected as cause and effect. Nor is this the only case that induces me to draw such a conclusion. On the 26th of February, 1810, I opened the body of a horse, that had died of tetanus, and had been cut for the hooks. I found the right lobe of his lungs in a high state of inflammation as well as the whole intestinal canal. In certain cases of this disease all the common symptoms of fever are to be met with. I have for these reasons long viewed the disease as both acute and chronic and have thought that the slow or chronic is the

least to be dreaded. I imagine that the disease proceeds to a more rapid termination, when it proceeds from fever and the reverse. It is remarkable in tetanus from wounds that the wounded part always heals long before the symptoms of locked jaw appear, in many from five or six days to two and even three weeks afterwards.—I have always noticed that when the spasms were communicated to the intestines that the chance was a bad one, and that an obstinate constipation was equally unfavourable. A free passage, especially when not produced by art, is I believe, always favourable.—Since you recommended the use of mercury, I have cured three cases out of five, and I am of the opinion, that the fourth would have done well, had not the owner of the horse contrary to my advice, permitted the operation for the hooks to have been performed. The fifth was one of those desperate cases, and so far advanced, that I think no remedy could have had any effect. I could not perceive that the mercury had any effect upon the excretions, or any other, except that of relaxing the spasms in a few hours after it was given. The glands were not affected that I could perceive, as no part of the mouth, fauces or tongue shewed any marks of its action. The intestines were little, if at all affected, as no purgative effects were perceptible.*

Perhaps it would be useful to state a case exhibiting in general the mode of treatment I have used in those cases where mercury proved successful.

On the 22d March, 1810, I was called to see a horse supposed to have had the hooks, and as the owner pretended to no knowledge of the disease, he had through the persuasion of others suffered the operation to be performed several days before I saw him. Upon enquiry I found that a bone had entered one of his feet about three weeks before; but had been extracted a day or two afterwards, and left little or no lameness. For this reason those who saw him could not be per-

* I should have remarked that the intestines almost in every instance very soon partake of the spasmodic affections, and that this is a very hard state to overcome.

suaded that the disease could have arisen from this cause. It was easy to be perceived that the symptoms of locked jaw were increasing, but as they had proceeded gradually my prospect was pleasing. The wounded part was dilated immediately and dressed with cantharides and spirit of turpentine. The symptoms did not abate and the evident marks of strong spasmodic action rendered something farther necessary. I with great difficulty introduced into the mouth six drams of calomel. On the 23d the calomel was repeated. 24th calomel repeated, but no appearances of relief. 25th calomel repeated, and the spasms less frequent and of shorter duration. 26th spasms relieved. This morning he passed from the rectum a small watery discharge, but nothing like a free or active purgation. 27th, he is better, and can distend his jaws considerably, but has no appetite. I gave an injection of corn meal gruel. 28th, purges a little, and the injection was repeated. 29th, no more passage from the intestines. His jaws quite free, and appetite returning. He soon recovered, and is now a very fine animal.

Your obedient servant,

JOHN HASLAM.

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Remarks on Hydrophobia, and a new method of using the Caustic by H. Wilkins, M. D. in a letter to the Editor.

SIR,

AS I conceive that any improvement in the manner of treating hydrophobia, will be acceptable to the public, I shall make known through the Lycæum, the simple and efficatious method I have used, and recommended for some years, to eradicate the dreadful poison that occasions it. A method not altogether new in substance, though it be new in form, and if applied with a little attention, would reduce this once tremendous disease to the lowest degree on the scale of mortality.—Before disclosing my plan, permit me to make a few

observations, that relate to the great importance of destroying the virus in its first lodgement; for I consider all other plans of prevention as merely experimental, and deviating from true wisdom.

As to the medicines, called specifics, or family medicines, I regard them in toto as the base offspring of ignorance, or avarice, and not worthy an examination: a gloomy experience having long since decided against them. With regard to these it is worthy of remark, how error begets error. I have known a kind of hawk-weed, (Hyoseris Amplexicaulis,) given with the greatest applause under an idea that it was the litchen caninus, though not even belonging to the same class of plants; by which we have gained the knowledge that H. A. is a diuretic and may be safely given in as large doses as the litchen, but surely there never was a human being saved from hydrophobia by it!—And I have no doubt but many a little running plant has been swallowed for the famous anagallis, hereafter to be remembered for having arrested the attention of the legislature of Pennsylvania, more than for all its pretended cure.

The well authenticated histories of hydrophobia leave no room to doubt, that scarce any period, within a year from the infliction of the wound, is too * long for a local affection to exist nor too short † for it to expand into the dreadful features that characterize the disease. How far this variation depends upon the irritability of the general habit, the local irritability of the part wounded, or the sympathetic disposition of the wounded organ with the parts afterwards associated in diseased action; or the disposition of the wounded part to suppurate; I will not take upon me to determine: but I believe them concerned in determining, both the certainty and the time of the disease.

* Dr. Vaughans cases and observations on hydrop. page 22, a case after nine months.

† Dr. Grays case on India, where death followed in the course of the same day.

When we consider a period so long as nine months from the insertion of the virus to the production of the disease, with an intervening state of apparent health; as in the case alluded to above, the least consideration will lead us to the conclusion, that the local disease must be first formed about the wounded part; and that, the disease remained there in a state of subexistence: for we cannot suppose that a drop of poisoned saliva could remain unabsorbed so long, nor remain without generating new matter; nor that it could in a slight skin wound, (which will often produce the disease,) send forth irritations, after the manner of tetanus, to produce such a set of symptoms, as characterize the disease in question. It must therefore be gaining strength by generating new matter, till it be sufficiently powerful to derange all the natural actions.—With respect to the poison laying local and first infecting the place of deposit, though not peculiar to this disease, it may be difficult to explain satisfactorily the pathology, yet, on this dark and and intricate subject, I have often pacified, if not satisfied my mind by adverting to the doctrine and to the fact, which takes place in vegetation. It is well known, that after cutting down certain vegetables, as grasses for instance, another kind will spring up, and in some instances fir-trees have been raised, by cutting off the incumbent timber, where they had been unknown for a long series of years. There can be no doubt, but that a degree of vegetation had constantly gone on, though not in full form, owing to the reigning crop which had kept them in a state of subexistence: certain fibrillæ and gangliæ being discoverable under ground in the case of the firs alluded to above.

In this way I conceive does vigorous health keep under various morbific seeds, which are ready to germinate upon the least overthrow of the general healthy state, and then put on the full characteristic form by which we know and name them.

A question might be here propounded whether the materies of hydrophobia does dot act simply from irritation as

a wound from a rusty nail produces lock-jaw?—There seems to be a possibility of this, when we consider the rapid movement of the case in India by Dr. Gray as quoted above; but I think it inquestionable, that it gains strength and quantity at the part, before it is discharged in sufficient power to cope with and overcome the natural actions; for it is by altering these and instituting new actions, that the disorder is formed.

Was it not for the power of communicating the identical desease, we might be greatly puzzled to determine whether there was any morbific matter, or any difference in the mode of action from that in lock-jaw; viz. by simple irritation at the wound, affecting certain parts, according to the relation that such irritation bore to such or such parts of the system.—I would presume to judge that in so rapid a case as the one alluded to above, there could scarcely be any more morbific matter formed than what was first inserted.

After what has been said, I ask the question, how can the mind be turned from the purpose of making a local application, that will destroy the first diseased part, and perhaps decompose the poison itself, so that if left in the wound it might not act? There an application is always proper, for it can never do harm, and if the symptoms are not yet discoverable that characterize a general disorder, we are morally sure of success.

Here the time that the disease takes to form itself, is of no consequence, unless it be the longer the more favourable.—It is worth while to examine well a method of this kind and for a fair elucidation, to compare it with what is called a specific; because I do not insist, that such a discovery may not be hereafter made. E. G.—A and B are bitten at the same time, and by the same dog—A. in the common course of things would have the disease in nine days, B. not till nine weeks or months. Let them both be treated at any anterior time with the caustic (hereafter recommended) and both are surely cured.—But let them take the internal specific for a month: A. is cured, and B. is supposed to be also

cured, but the disorder is yet local, when the specific is laid aside, and behold at an unexpected time, a time of ideal security, the disorder spreads insidiously forth in all its terrible forms to invalidate the cure in A. and destroy B. What is now to be done? Another specific is wanted; a specific cure to aid the specific preventive! Let credulity itself blush for such hopeless contingencies, and adopt sound sense. But should not the specific medicine have been continued a longer time? Yes, a man may eat medicine for a year, but it would only keep him sick, and of course in constant fear, if it had any powers in it; and the physician must be ignorant indeed that does not know the difficulty of enforcing a long continuance of any drug.

To delay no longer, the method I recommend is as follows—Take ʒij. of hard vegetable caustic or caustic of potash, beat it fine, drop it in water and rub it till it becomes of the consistence of paint, such as is used for house painting. Dip a camel's hair pencil, or if that cannot be procured, a piece of fine linen rolled up hard, in the manner a mop is made, and thrust this paste into every sinus of the wound. Wait about one minute, when a shining granulous appearance shews that it has corroded the parts, then with a small syringe force in some weak, warm vinegar, which will clean out the wound and destroy the caustic. If the wound be considerably enlarged, no more need be applied, but if it is not, apply it again as at first and wait a little longer, when if the wound is cleaned out as before, it will be about as large again as it was before the application was made. This cleaning out of the wound may be notwithstanding my former supposition, all important, for what will it avail to destroy the part and leave the matter there? This points out the excellency of common caustic over lunar caustic. Nothing farther remains than to dress the wound with a good suppurative salve, such as merc: præcip: rub: levigat: ʒiij: cum unguento Basilic: ℥i.

The pain produced by the caustic paste is very trifling; children bear it without crying. In one instance I applied

it to twelve wounds on a boy of ten years of age and he seldom changed countenance or shewed signs of pain. In this case I had full proof of the success of the caustic; that it was a genuine case, there remains no doubt, for a horse and cow were bitten at the same time by the same dog, and both these died, whilst the boy is alive and well.

I could enumerate many instances where I have recommended, or applyed the caustic with success, but I trust the method wants no supports. It is safe, easy and sure. I have mentioned above, that exclusive of removing the diseased part, there was a strong probability that the materies was decomposed by the chemical action of the caustic, so as to be inert, even if left in the wound. This probability (though never to be trusted) is farther confirmed by the digestive powers having at times a similar effect.

I will relate a well authenticated case, as it is singular in the extreme, that may throw light on this, as well as on other points of this interesting disorder. A mad fiste went to a farm five miles from Baltimore, and there bit one of the domestic dogs, after which the fiste was killed. A very poor dog belonging to some of the blacks on the same farm, was seen by two gentlemen who had visited the house, eating the dead fiste. They were shocked, and attempted to drive him off, but he seemed so unwilling to leave it, that they left him. In a short time they returned by the same road, and found that the whole head had been eaten off. I have made enquiry (having daily opportunities) and find that the dog bitten died mad, but that the carnivorous dog is actually in better health.

The case of Admiral Rowley's son, might be brought as an objection that caustic was not infallible. The lip is certainly a bad part of the mouth, and in such cases a tooth may pass inside and not be noticed. Lunar caustic, is, moreover, very unfit for the purpose; and the person, recommended by Mr. Hunter, might have been very much off his guard by the manner in which the youth was attacked *by disturbing a*

*strange dog at his meal, and unknown to be diseased by his owner.**

When the mouth is bitten, as is often the case in children who are generally crying at the moment the attack is made, the gums, tongue and every contiguous part should be well examined.

Although I have avoided giving a description of the usual symptoms that characterize hydrophobia, as being useless at this time; yet I conceive it highly proper to notice any symptoms that have escaped general notice; and I would here note particularly, the intoxicated deportment and appearance about the eyes and a croup like sound in barking, as symptoms of hydrophobia. Hence the reason why patients in this disease have been said to have barked like dogs; for there seems to be an affection of the trachea, or gluttis which alters the sound in both the human and canine species.

Before closing this, I would caution every person against considering the dread of water as unequivocal. I have seen two instances where they drank freely and yet communicated the disease.

I do not think it proper whilst we are so far from perfection in the knowledge of this disease, to alter the general character, because in a few instances it is defective, and in this I am sanctioned by a great character, no less than Linneus.

Hoping that these observations may be useful, and lead to an uniform plan of prevention,

I remain, sir, yours, &c.

H. WILKINS.

P. S. Would not the paste caustic answer completely in shallow fistulas to remove the callous edges, and be a very easy mode of operating both to physician and patient?

* Annual Register, January 19, 1805.

MEDICAL

And Philosophical Intelligence.

AN ACT

FOR FOUNDING A MEDICAL COLLEGE

In the City or Precincts of Baltimore, &c. &c.

.....

An Act for founding a MEDICAL COLLEGE, *in the city or precincts of Baltimore, for the instruction of Students in the different branches of Medicine.—Passed November Session*, 1807.

WHEREAS it appears to the General Assembly, that many benefits would accrue, not only to the State of Maryland, but to many other parts of the United States, from the establishment of a Seminary for the promotion of Medical knowledge, in the city of Baltimore—Therefore,

2 *Be it enacted by the General Assembly of Maryland*, That a College for the promotion of Medical knowledge, by the name of "THE COLLEGE OF MEDICINE OF MARYLAND," be established in the city or precincts of Baltimore, upon the following fundamental principles, to wit: The said College shall be founded and maintained forever upon a most liberal plan, for the benefit of Students of every country and every Religious denomination, who shall be freely admitted to equal privileges and advantages of education, and to all the honors of the College, according to their merit, without requiring or enforcing any religious or civil test, or

urging their attendance upon any particular plan of religious worship or service; nor shall any preference be given in the choice of a President, Professor, Lecturer or other Officer of the said College, on account of his particular religious profession; but regard shall be solely paid to his moral character, and other necessary qualifications to fill the place for which he shall be chosen.

§ 3. *And be it enacted*, That the Members of the Board of Medical Examiners for this state, for the time being, together with the President and the Professors of the said College, and their successors, shall be and are hereby declared to be one Community, Corporation and Body Politic, to have continuance forever, by the name of the "*Regents of the College of Medicine of Maryland.*"

§5. *And be it enacted*, That the said Regents and their successors, by the same name, shall be able and capable to enjoy to them and their successors in fee or for any less estate or estates, and lands, tenements, annuities, provisions or other hereditaments within this state, by the gift, grant, bargain, sale, alienation, enfeoffment, release, confirmation or devise of any person or persons, bodies politic or corporate capable to make the same, and such lands, tenements, rents, annuities, pensions, or other hereditaments, or any less estates, rights or interests of or in the same, at their pleasure to grant, alien, sell und transfer in such manner and form as they shall think meet, and convenient for the furtherance of the said College; and, also, that they may take and receive any sum or sums of money, and every kind, manner or portion of goods and chattels, that shall be sold or given to them, by any person or persons, bodies politic or corporate, capable to make a gift or sale thereof, and employ the same towards maintaining the said College, in such manner as they shall judge most necessary and convenient for the instruction of Students in Medicine and the sciences connected with it.

§ 5. *And be it enacted*, That the said Regents and their successors, shall be able in law to sue and be sued, plead and be impleaded in any court or courts, before any judge, judges,

or justices within this state and elsewhere, in all and all manner of suits, pleas, causes, matters and demands of whatsoever kind, nature or form they be, and to do all and every other matter and thing hereby contemplated to be done, in as full and effectual a manner as any other person or persons, bodies politic or corporate, in like cases may or can do.

§ 6. *And be it enacted*, That the yearly value of the messuages, houses, lands, tenements, rents, annuities, or other hereditaments and real estate of the said College and Corporation (exclusive of the lot and buildings occupied by the institution) shall not exceed thirty thousand dollars; and all gifts to the said College and Corporation, after the yearly value of their estates shall amount to thirty thousand dollars aforesaid, and all bargains and purchases to be made by the said Corporation, which may encrease the yearly value of said estate above or beyond the amount aforesaid, shall be absolutely void and of none effect.

§ 7. *And be it enacted*, That the said Regents and their successors, shall have full power and authority to have, make and use one common and public seal, and likewise one privy seal, with such devices and inscriptions as they shall think proper, and to ascertain, fix and regulate the uses of both seals, by their own laws, and the same seals, or either of them to change, break, alter and renew at their pleasure.

§ 8. *And be it enacted*, That the said regents shall have full power and authority to appoint a President of the College, who shall preside at their meetings, and perform such other duties as may be assigned to him; and in his absence to appoint a Vice President, which latter shall always be one of their own body.

§ 9. *And be it enacted*, That the said Regents shall from time to time, and at all times hereafter forever, have full power and authority to constitute and appoint, in such manner as they shall think best and most convenient, Professors of the different branches of Medicine, for instructing the Students of said College, by regular lectures upon every part of that science, who shall be severally stiled Professors

of such branches as they shall be nominated and appointed for, according to each particular nomination and appointment; and also to constitute and appoint in like manner, Lecturers upon the sciences subservient to or connected with Medicine, who shall be severally stiled Lecturers on such sciences as they shall be appointed for; and the said Professors and Lecturers so constituted and appointed from time to time, shall be known and distinguished forever as one learned body or faculty, by the name of the *Medical Faculty of the College of Medicine of Maryland*, and in that name shall be capable of choosing their own Dean of Faculty and of exercising such powers and authorities as the Regents of the said College, and their successors, shall by their ordinances think necessary to delegate to them, for the instruction, discipline and government of the said institution, and of all Students, Officers and Servants belonging to the same, provided that nothing be done, in virtue of this, contrary to the constitution and laws of this State, or to the constitution and laws of the United States.

§ 10. *And be it enacted*, That the said Regents and their successors shall meet at least once a year in stated annual meetings, to be appointed by their own ordinances and at such other times as by their said ordinances they may direct, in order to examine into all matters touching the discipline of the institution, and the good and wholesome execution of their ordinances; and that the said Regents, when duly assembled, shall have full power and authority to make their own rules of proceeding and to make fundamental ordinances for the government and discipline of the College, and to appoint the necessary officers of the same: in all which meetings a majority of the whole number of Regents shall be a quorum to do any business, except to vacate the seat of the President, Professor or Lecturer, for which purpose the consent of two thirds of the whole number of Regents shall be necessary.

§ 11. *And be it enacted*, That the Medical faculty of the said College, shall hold one term in each year, which shall

commence on the first Monday in November, in every year, and shall continue not less than four, nor more than six months, as may be regulated by the ordinances of the College; within which period all the Lectures to be delivered in the said College, shall be given, except the Lectures on Botany, and such other Lectures as the Regents shall judge will be delivered with more advantage at a different season.

§ 12. *And*, for animating and encouraging the Students of said College, to a laudable diligence, industry and progress in Medical science, *Be it enacted*, That the said Regents and their successors, shall, by a written mandate under their privy seal and the hand of their President or Vice President, have full power and authority to direct the Medical faculty of the said College, to hold Public Commencements, either on stated annual days, or occasionally, as the future ordinances of the said institution may direct; and at the said Commencements to admit any of the Students of the said College, meriting the same, (whose names shall be severally inserted in the said mandate) to the office and profession of Surgeon, or to the degree of Batchelor or Doctor of Medicine; and it is hereby enacted, that the President or Vice-President shall make out and sign with his name, diplomas and certificates of the admission to such offices or degrees, which shall be sealed with the Public or Greater Seal of the College or Corporation, and delivered to the Graduates as honourable and perpetual testimonials of such admission; which diplomas or certificates, if thought necessary for doing greater honour to such graduates, shall also be signed by the names of the different Professors or Lecturers, or as many of them as can conveniently sign the same: *Provided always*, That no Student or Students, within the said College, shall be admitted to any such offices or degrees, or have their names inserted in any mandate for that purpose, until such Students shall have been first duly examined and thought worthy of the same at a Public Examination of Candidates, to be held previous to the day of Commencement, in the said College, by and in the presence of the Regents

and other persons, choosing to attend the same; and shall also have undergone such previous private examinations, and have performed such exercises as shall be prescribed by the future ordinances of the College: *And provided further*, That no Student or Students in the said College, shall be admitted to the degree of Doctor of Medicine, unless he or they shall have attended Lectures in the said College during two Terms, and shall, during that period, have attended each of the Lectures prescribed by the ordinances, for at least one term: and shall also have written and caused to be printed, a thesis or theses in the Latin or English language, and shall publicly defend the same on the day of Commencement: But the Regents of the College shall, at any time, have power to consider the having attended Lectures in any other Medical Seminary of established reputation, and for equal space of time, as equivalent to having attended one of the Terms above prescribed.

§ 13. *And be it enacted*, That the Regents shall, at any time, have the power of conferring the honorary degree of Doctor of Medicine on any Physician, who has practised Physic for twenty years, within the State of Maryland; and of conferring the honorary degree of Batchelor of Medicine on any one who has practised Physic for ten years within the same.

§ 14. *And be it enacted*, That every Licentiate of the Board of Medical Examiners, who shall have practised Physic for five years within this State, shall have a right to demand and receive from the College aforesaid, a Surgeon's Certificate, free of all expense, except the sum of one dollar to the Register or other such officer of the College for his trouble in making out the same.

§ 15. *And be it enacted*, That the right of the said College to confer degrees, shall not take place until Professors shall have been appointed, and one course of Lectures delivered upon the several branches of Anatomy, Chemistry, Materia Medica, and the practice of Physic: *Provided*, That nothing in this act shall be construed to prohibit the said Regents

from conferring, at any time, the honorary degrees before mentioned; or from admitting at any time, to the degree of Doctor of Medicine, such persons as may be appointed Professors in the College, on their performing such Exercises and duties as may be prescribed by the ordinances.

§ 16. *And be it enacted*, That until further arrangements be made by the Regents of the said College, *John B. Davidge*, *M. D.* and *James Cocke*, *M. D.* shall be joint Professors of Anatomy, Surgery and Physiology; *George Brown*, *M. D.* shall be Professor of the Practice and Theory of Medicine; *John Shaw*, *M. D.* shall be Professor of Chemistry; *Thomas E. Bond*, *M. D.* shall be Professor of Materia Medica, and *William Donaldson*, *M. D.* shall be Professor of the Institutes of Medicine; and the said Professors, or any three of them, shall appoint the time and place of the first meeting of the Regents of the said College, giving one week's notice of it in two newspapers published in the city of Baltimore.

§ 17. *And be it enacted*, That all Students who matriculate in the said College, previous to the first day of January, and attend any of the Lectures therein, to the end of the course, shall be considered as having completed a Term.

§ 18. *And be it enacted*, That the *Medical and Chirurgical Faculty* of the State of Maryland, shall be considered as the patrons and visitors of the said College; and their President, for the time being, shall be Chancellor of the College; and the Medical Faculty of the said College, shall give in to the said Medical and Chirurgical Faculty, at each of their biennial meetings, a report of the progress of learning, in the said College, and of such other particulars as they shall think fit to communicate.

§ 19. *And be it enacted*, That in case, at any time hereafter, through oversight, or otherwise through misapprehension and mistaken construction of the powers, liberties and franchises in this Charter or Act of Incorporation, granted or intended to be granted, any ordinance should be made by the said Corporation of Regents, or any matters done and

transacted by the Corporation, contrary to the ten or thereof—all such ordinances, acts and doings shall, of themselves, be null and void: Yet they shall not, in any Court of law, or by the General Assembly, be deemed, taken, interpreted or adjudged into an avoidance or forfeiture of this Charter and Act of Incorporation; but the same shall be and remain unhurt, inviolate, and entire, unto the said Corporation of Regents, in perpetual succession; and all their acts, conformable to the powers, true intent and meaning hereof, shall be and remain in full force and validity, the nullity and avoidance of such acts to the contrary notwithstanding.

§ 20. *And be it enacted*, That this Charter and Act of Incorporation, and every part thereof, shall be good and available in all things in the law, according to the true intent and meaning thereof; and shall be construed, reputed and adjudged, in all cases, most favourable on behalf and for the best benefit and behoof of the said Regents and their successors, so as most effectually to answer the valuable ends of this Act of Incorporation towards the general advancement and promotion of Medical knowledge.

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I CERTIFY, That the aforegoing is a true copy from the original, passed both branches of the Legislature, on the eighteenth day of December, one thousand eight hundred and seven.

Test. JOHN BREWER, Clk. H. Del.

College of Medicine, of Maryland.

Courses of Lectures will be delivered in this Institution next winter on the following subjects:

1. On Anatomy, Surgery, and Physiology, by John B. Davidge, M. D. and James Cocke, M. D.
2. On the Practice and Theory of Medicine, by Nathaniel Potter, M. D.
3. On Chemistry, by Elisha De Butts, M. D.
4. On Materia Medica, by Samuel Baker, M. D.

The courses of Lectures will commence on the first Monday in November, 1811.

By order of the President,

JAMES COCKE, Sec'ry.

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Licentiates admitted to practice Physic and Surgery, in the State of Maryland, at the meeting of the Board in June, 1811.

Jonathan Waters,	Benedict I. Semmes,
Robert Dawson,	I. B. Taylor,
John S. Stockett,	William N. Luckey,
Henry Stonestreet,	Corbin Amos,
Stanislaus Coomes,	Charles D. Bruce.

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At a meeting of the Regents of the University, held at New York, pursuant to adjournment, in the Senate Chamber, on the 1st of April, 1811:

Resolved, That the following appointments be and hereby are made in the College of Physicians and Surgeons in the city of New York, to wit:

Samuel Bard, M. D. President,

Benjamin De Witt, M. D. Vice President.

Wright Post and John Augustine Smith, Joint Professors of Anatomy, Surgery and Physiology.

David Hosack, M. D. Professor of the Theory and Practice of Physic and Clinical Medicine.

William James Mac Neven, M. D. Professor of Chemistry.

John R. B. Rodgers, M. D. Professor of Obstetrics, and the Diseases of Women and Children.

Edward Miller, M. D. Professor of Therapeutics, and Clinical Medicine.

William Hamersley, M. D. Professor of the Institutes of Medicine.

Samuel L. Mitchell, M. D. Professor of Natural History.

James S. Stringham, M. D. Professor of Medical Jurisprudence.

Archibald Bruce, M. D. Professor of Mineralogy and Pharmacy.

John D. Jaques, M. D. Treasurer.

John W. Francis, Register.

And that all former appointments made in the said College of Physicians and Surgeons be, and the same are hereby revoked.

A true extract from the minutes,

FR. BLOODGOOD, Sec'ry.

Translated from the Bulletin de la Societe de la Medicine.

DEATH OF FOURCROY.

THE medical faculty was yet deploring the loss of several of its professors, when a new object of affliction was added to the mass of regrets, so profound and so recent. *Count Fourcroy* died suddenly, on the morning of the 16th December, 1809, of an apoplectic fit; at the tomb, *D. Thouret*, in the name, and as Dean of the Faculty of Physic, pronounced the following discourse:

Profound grief reigns in this assembly; from every side breaks forth regrets; what bounds, what limits can be given to them!

What a man hath death torn from us? What talents are this day swallowed up in the grave? A glory earlier than usual, had announced what he was going to be. It seems as if fate had foreseen that nature would circumscribe his career, and had willed him a recompense. Scarcely had he left the benches—what do I say—he still sat among us—when we already marked the place that he was destined to hold among the most illustrious men. In the first bloom of his years in the midst of his fellow students, he made attempts to excel in the science of that professorship which he has carried to so high a degree of perfection.

A pupil of the celebrated *Bucquet*, he soon became his rival; and the teaching of chemistry which that learned professor, as it were, bequeathed him, lost nothing of the splendour which its first master had given it.

Talent in him was animated with that strong passion which alone can produce entire success. He sought all opportunities of displaying it, at the same time that all the establishments vied with each other in affording them to him, and in soliciting his support.

Who can properly paint that indefatigable activity which rendered him equal to all his duties to all the labours of his